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From the Imaginary to the Real: The Back and Forth Between Reality and Simulation

Harvey Whitehouse, Ken Kahn, Michael E. Hochberg, and Joanna J. Bryson

We are grateful to all the respondents for their insightful commentaries. Many clearly welcome the dialogue we were hoping our article would facilitate between modelers and social scientists. The intention of our article was to present a case study that primarily has value in advancing a specific area of religious study, and secondarily value in discussing open methodological issues on the role and practice of social simulation in the humanities and social sciences. We thank our reviewers for their many observations and for improving and broadening the scope of our contribution with their arguments and many useful references to previous work.

We appreciate the efforts **Chattoe-Brown** and **McCorkle & Lane** have made to document previous work in their commentaries. We of course never claimed to be the first to use agent-based modeling (ABM) to study religious phenomena, nor to be offering a complete review of the technique. We also did not claim to be the first to say that a model is a well-specified theory, and in fact cited two texts that do provide more extensive reviews on this matter (Bryson et al., 2007; Kokko, 2007). It is worth noting though that a “growing consensus” on this approach is by no means an indication of unanimity. There are still many who hold with Axelrod’s (1997, p. 4) statement that ABM is “a third way to do science... Whereas the purpose of induction is to find patterns in data and that of deduction is to find consequences of assumptions, the purpose of agent-based modeling is to aid intuition. Agent-based modeling is a way of doing thought experiments.” **Czachesz** also provides a critique of contemporary researchers applying this approach. Those of us promoting the model-as-theory approach to ABM believe that this perspective both underestimates the utility of simulations and overestimates the formality and certainty of the standard scientific methods for deducing predictions from theories. Viewing model building as *exactly* theory building demands more rigor from the models while simultaneously facilitating the scientific process in novel ways. The discussion section we provide (“Recalibrating the DMR Theory”) is intended to contribute not only to the DMR theory but also methodologically to the scientific application of ABM, by demonstrating good practice in model outcome communication and by validating through demonstration the model-as-theory approach.

We thank Costopoulos and Hooper both for drawing attention to a more unusual feature of our paper – that we seek to bring together salient features of cognition, behavior, and cultural systems in our models rather than to treat these features in isolation. We agree in principle with **Costopolous** that this approach has the potential to go further still by providing novel insights into the mechanisms and processes of

selection governing sociocultural evolution. In practice, however, there is a computational cost to this complexity of modeling that affects not only the speed that a simulation can run but also the complexity of its results particularly where those are combined with learning and evolution. This is why the primary goal of Model 1 has been to use ABMs as a way of examining the internal consistency of our theories. Model 2 in contrast looks at long-term dynamics, but therefore is a far simpler model of the religion and agents. Both models are hybrids of course, looking at the dynamic functioning of features of a system both in place and over time, but each has different emphases to allow the detailed exploration of different parts of the problem.

By standard scientific practice a model / theory should only be exactly as complex as necessary to simulate / explain the phenomenon it intends to address. Some of our commentators seem uncomfortable with this basic aspect of scientific method. We can see this in the critique of the agents in our models raised in different ways by Geller and Czachesz. **Geller** argues that the semantic networks in Model 1 lack a motivational component and “since there is no pulse of life in the simulation, there is no purpose for these agents to reason in religious terms”. These comments, however, present something of a puzzle. Each node in the semantic network was explicitly accorded a motivational value that was a function of its combined psychological properties (emotional salience and intuitiveness) and its frequency of activation (motivational force declining as frequency increases). So we are at a loss to understand Geller’s point about lack of ‘purpose’ or ‘pulse of life’. **Costopoulos** is perturbed by the level of detail in our models and the number of unjustified assumptions. But there are no new assumptions here that have not been previously published – in fact, if anything there are fewer and the models are more detailed. It is unfortunately common that when scientists and philosophers see their ideas really spelled out in a detailed model they may not recognize them, seeing suddenly only the deficits, gaps and lack of detail. All models, indeed all theories and all science, take place at a level of abstraction. This is the goal of science, to abstract what matters – what is predictive of the future – from causally irrelevant (or less relevant) details of everyday life and the physical world. **Hooper** makes this point well. That a simulation brings out the complexity of the system and its assumptions is useful and can hopefully accelerate progress improving the DMR theory. This is not to say that there are no gaps or unwarranted assumptions in our model, or scope for alternative hypotheses. Of course there is much more work to be done beyond the contributions demonstrated here. Indeed several extensions and improvements have been proposed by other commentators on our paper (e.g. **McCorkle & Lane** and **Komarova**, see below).

According to **Czachesz**, there are conceptual problems with our attribution of motivation to agents. He suggests that these problems have been solved by the psychological research he cites, but does not specify what the problems are (still less how they have been overcome by psychologists). Later he returns to the issue of motivation: “If we set aside the conceptual problems with “motivation” (see above), there remains a serious concern with regard to its measurement in the model.” Thus,

the nature of the alleged conceptual problems is never stated, only set aside in order to address issues of measurement instead. According to Czachesz we have proposed that motivation levels regulate the stability of the group (we presume he means here the stability of the group's beliefs). Nevertheless, Czachesz continues, "what if fifty percent of the group has very high motivation, whereas the other fifty percent has very low motivation? This presents a perfect scenario for schism, or at least for serious inner conflict". It is not clear that this is a concern about *measurement* of motivation levels per se, but rather about what levels of uniformity of motivational states in a population can be assumed by the model. We have assumed that the continual repetition of ritual and creed over time gradually erodes motivation levels and does so quite uniformly with a given community as a whole (the so-called 'tedium effect', in support of which we cited empirical evidence). Owing to this uniformity, periodic efforts to advance unorthodox beliefs are crushed by consensus when motivation levels are high and, by the same token, embraced by consensus when motivation levels are low. Does this mean our model could not account for schism? We think not, because communities differ in terms of motivational levels even if internally they tend towards motivational homogeneity. If we were to enlarge the Kivung model to include not only groups undergoing splintering but other groups within the mainstream movement that are not, it would be obvious that splinter groups are schisms in that wider context (as the notion of 'splintering' of course implies). While we might debate the empirical evidence for these assumptions in our model, we think Czachesz's concerns about the way we measure motivation are unfounded.

The commentary by Czachesz raises a number of other issues that would seem to reflect misunderstandings. For instance, he argues that recent developments in the study of memory have cast doubt on the cognitive foundations of the modes theory. Although Czachesz does not say what these recent developments are, or why they pose problems for the modes theory, he argues that they result in our 'minimizing' the role of memory in our theories of both doctrinal and imagistic dynamics. With regard to the doctrinal mode, what we actually say in the article is that "high-frequency ritual performances allow complex networks of ideas to be transmitted and stored in semantic memory". In the model itself memory is one of our key parameters: "the frequency of exposure to a given node in the network will impact both memory and motivation: as frequency increases the risk of forgetting is reduced but so too is emotional salience; as frequency decreases, garbling and forgetting become more likely but emotional intensity is enhanced". With regard to the imagistic mode we observe, "Such practices trigger enduring and vivid episodic memories for ritual ordeals, encouraging long-term rumination on the mystical significance of the acts and artefacts involved... Traumatic rituals create strong bonds among those who experience them together, establishing in people's episodic memories exactly who was present when a particular cycle of rituals took place." These claims with regard to the effects of memory on the production and transmission of religious beliefs, norms, and practices have been a stable feature of the modes theory since its inception (Whitehouse 1992, 1995). Moreover, these claims were systematically integrated into our models. Czachesz's comments suggesting

otherwise are hard to interpret. What might be more to the point is that Model 1 would need to be expanded to take account of the longevity of episodic recall for low-frequency, high-arousal rituals, enabling us to ensure that recurring imagistic outbursts produce novel semantic networks rather than repeating earlier ones (a point to which we return in our concluding paragraph).

Czachesz maintains that “empirical work has not confirmed... predictions about the imagistic mode”, a claim that rests on a single publication (Konvalinka et al., 2011). The study cited measured heart rates among a range of observers and participants in an emotionally arousing ritual (fire walking) and concluded that levels of emotional arousal (using heart rate as a proxy) were equally high among fire walkers and those members of the audience related to them (by kinship or close association) but not among audience members who were unrelated, or more distantly related. This is an interesting study but certainly does not disconfirm any predictions of the modes theory. If the claim merely that it ‘has not confirmed’ it either, this is a rather odd statement because the study in question was not intended to test predictions of the modes theory.

Although many of the points raised by Czachesz left us somewhat puzzled, we welcome his query about whether the doctrinal-imagistic oscillation in the Model 1 is generalizable to other religions (he asks whether it applies generally to ‘pre-state’ societies but that is not really a relevant question since it has never been proposed that doctrinal traditions and patterns of splintering within them are found particularly or only in ‘pre-state’ societies). A central goal of our article was to show how models could be used to explore the implications of evidence-based theories. So it is entirely appropriate to ask where the evidence for our modeling assumptions comes from. We cited evidence for the generalizability of the oscillation simulated in Model 1, but a more general articulation of Czachesz’s concern is provided by **Chattoe-Brown** when he writes: “It may be that the authors are very clear what are data, what are assumptions (needed to make the model ‘work’ absent data) and what are results, but unless the skeptical ‘subject specialist’ is kept excruciatingly clear about these distinctions, they may draw unfavourable conclusions.” Chattoe-Brown’s suggestion that we tag each assumption of a given model as either supported by data (sources cited) or currently unsupported is well taken. Perhaps this should be standard practice in all efforts to model complex social phenomena. From the viewpoint of presentation such a practice might seem inelegant (hence ‘excruciating’) but clarity and precision should no doubt come before style and readability.

The simulations reported in our article are focused largely on proximate mechanisms rather than on questions of function, adaptation, and selection. Hooper encourages us to address the kinds of resource extraction problems that the two modes of religiosity might resolve. **Chattoe-Brown** similarly urges us to consider the ecologies of groups in our simulations. We agree that these are important issues and plan to tackle them directly in the next wave of models to be developed (though see our earlier comments on complexity). The work presented in the current paper is merely a foundation on

which to build, as demonstrated by some of our commentators who have done so. Nevertheless, **Hooper**'s comments on the kinds of issues at stake are remarkably penetrating, as for instance where he observes: "we might speculate that doctrinal practices facilitating norm enforcement (e.g. top-down pedagogy, religious courts) may be better suited to achieving cohesion at the scale of, say, world religions, while imagistic practices promoting social bonding (e.g. mutilating rituals) may be better suited for bringing together smaller, more intimate communities." Indeed, we may soon do more than speculate since we are gathering increasingly systematic evidence in support of this prediction, not only from comparative ethnography but also from archaeology, historiography, and from a host of new experiments and surveys investigating the relationship between ritual and the scale and structure of social groups (<http://www.cam.ox.ac.uk/ritual/>).

A key strand of our argument, echoed by **Costopolous**, was that one of the considerable benefits of modeling is to establish whether our theories are internally consistent. Sometimes lacunae and gaps in our theories dawn on us for the first time in the process of designing models and sometimes only when analyzing the results. But it can also happen that such problems are identified and debated for the first time after the models are published. This could hardly be demonstrated more clearly than in the commentary by **McCorkle & Lane**. They point out that our model of the oscillation between mainstream and splinter group systems in Model 1 is incomplete. And they demonstrate one way of filling in that gap by modifying the code. This encourages us to attend not only to issues of internal consistency in the model but to questions about what is happening in the real world when cults collapse but cardinal beliefs persist. Moreover, by taking a longer-term view of these oscillations, as McCorkle & Lane do, it becomes clear that historically documented innovations in successive splinter groups must be informed by memories of previous ones and so future instantiations of Model 1 would need to reflect this. There is, and should be, a two-way street between research in the real world and modeling in a simulated environment. It is this potential for a productive back-and-forth between reality and simulation that we hope our article, and the commentaries it has provoked, will convey to modeling agnostics.

We were particularly impressed and excited by the improvements and extensions made directly to our models and theories by **Komarova** as well as McCorkle & Lane. Drawing on the tools of theoretical and population biology, Komarova not only reveals alternative methods of modeling similar patterns of transformation in religious systems but perhaps even more profoundly the possibility that similar mechanisms can emerge in different evolutionary systems. The parallels Komarova postulates between imagistic dynamics and colonies of cancer cells exhibiting the 'mutator phenotype' are striking and potentially open up new questions about the nature of activist religions in general (and cargo cults in particular), for instance concerning patterns and rates of spread as compared with missionary teachings exhibiting much lower rates of mutation. What might seem a fanciful analogy could turn out to be a novel insight into processes of religious evolution when a common mechanism is specified with the mathematical

precision that modeling provides. These encouraging and constructive results even in the brief period allowed them for the preparation of commentaries demonstrate the utility of scientific communication through open-source model sharing. A common pattern in the scientific use of ABM is the redescription where possible of the simulation findings more formally in mathematics. Where multiple methods and tools for reasoning can be brought together in this way and their results correlate, we can have greater confidence that we are making progress theoretically.

References

Axelrod R. (1997) *The Complexity of Cooperation: Agent-Based Models of Competition and Collaboration*, Princeton NJ, Princeton University Press.

Bryson, Joanna J., Ando, Yasushi, and Lehmann, Hagen. (2007). Agent-based models as scientific methodology: A case study analysing primate social behavior. *Philosophical Transactions of the Royal Society, B — Biology* 362 (1485):1685–1698.

Kokko, Hann. (2007). *Modeling for Field Biologists and Other Interesting People*. Cambridge University Press.

Konvalinka, I. Xygalatas, D., Bulbulia, J., Schjødt, U., Jegindø, E., Wallot, S., Van Orden, G., & Roepstorff, A. (2011). Synchronized arousal between performers and related spectators in a fire-walking ritual. *Proceedings of the National Academy of Sciences* 108 (20), 8514-8519.

Whitehouse, H. 1992 "Memorable Religions: transmission, codification, and change in divergent Melanesian contexts" in *Man* (N.S.) Vol. 27, No. 3: 777-797.

Whitehouse, H. 1995 *Inside the Cult: religious innovation and transmission in Papua New Guinea*, Oxford: Oxford University Press